

conception of that bone, viz. that it is of costal origin, but the evidence on which he bases his conclusions is not convincing. For three of the studies Prof. Young is either in part or wholly responsible, and he is to be congratulated on the vigour shown by the Manchester school of anatomists.

*Refraktionstafeln.* By Dr. L. de Ball, Direktor der v. Kuffnerschen Sternwarte. Pp. xiv+18. (Leipzig: W. Engelmann, 1906.) Price 2.40 marks.

THE methods of computing corrections for atmospheric refraction have always been more or less unsatisfactory. The conditions of the problem do not lend themselves to extreme accuracy on account of the uncertainty of the meteorological elements introduced. The determination of the density of the atmosphere at any precise moment, dependent as it is on the temperature, the amount of aqueous vapour present, and other conditions, is not simple, and custom and authority alike have sanctioned the employment of rough and approximate data. Bessel's tables, so long in use, were admittedly founded upon inadequate material, and probably would have long since been superseded but for the inconvenience that arises when any breach of continuity occurs in a long series of observations; but in observatories where measures of zenith distance have been made at small altitudes this inconvenience has had to be faced. At Greenwich, for example, corrections to Bessel's tables, or Airy's modifications of them, have been alternately introduced and rejected in the treatment of observations at large zenith distances.

In the tables which Dr. L. de Ball has issued the difficulty of continuing an unbroken series of corrections, available from the zenith to the horizon, has not been attempted. The tables as arranged are available up to  $75^\circ$  zenith distance, and within this limit represent a consistent theory, that of M. Radau. The form in which the tables are constructed gives the log. of the refraction presumably correct to four places of decimals. In the example worked out it has been necessary to take out five significant integers, and, if the second decimal place is to be correct, this may be rather a severe strain on four-figure logs.; but Dr. L. de Ball gives very good and sufficient reasons for not extending the tables beyond these limits. He reminds us that the determination of the temperature of the air is not so easy as the reading of a thermometer seems to suggest. The thermometer bulb is affected by the heat rays emitted by the objects which surround it, whilst the air absorbs only a part of those rays. On these grounds the temperature indicated by the thermometer may easily differ  $0.2^\circ\text{C}$ . from that of the atmosphere, and such a difference would occasion an error of three units in the fourth decimal of the log. of the density, and a similar amount in the log. of the refraction. The tables aim at giving an accuracy which is sufficient and practical rather than making a claim to extreme and misleading rigour. A further proof that the author has considered the practical side is shown by the fact that he has included tables designed to assist the computation of differences of refraction, applicable to the reduction of heliometer and photographic observations.

*The Butterflies of the British Isles.* By Richard South, F.E.S. Pp. x+204. (London: Frederick Warne and Co., 1906.) Price 6s. net.

NOTWITHSTANDING the large number of books relating to British butterflies, there was still room for a pocket handbook which should do for the present generation what Coleman's "British Butterflies" did for the last, and this want Mr. South has set himself to provide. He has succeeded in giving us a portable little book, well up to date, containing full

information about structure, transformation, setting, &c., besides a good account of the individual species. The plates contain coloured figures of the butterflies on one side of the page, and plain figures of caterpillars, &c., on the back, thus doubling the number of page illustrations without adding to the thickness of the book. The illustrations in the text are nearly all in the introduction. They are uncoloured, and some of them are taken from Sharp, Aurivillius, and other trustworthy authorities.

Mr. South admits sixty-eight species as British, but regards only fifty-seven of these as actual natives; but surely, though some of the remainder are extinct, and others only casual visitors, the black-veined white (once abundant, but now almost extinct in England), and the red admiral, still one of the commonest of the *Vanessidæ*, ought to have been included among the genuine natives. The evidence against the red admiral being a genuine British species seems to rest on the assumption of its being a migrant, though this is admittedly not proved, as it is abundantly in the case of its nearest ally, the painted lady.

The rapid disappearance of butterflies in England is doubtless largely due to the wholesale clearing away of the weeds and plants on which the caterpillars feed, by the utilisation of every scrap of waste ground. Yet this cannot be the only reason, or the black-veined white, which feeds on hawthorn as well as on fruit trees, would not be disappearing. In this case the disappearance of the butterfly seems to be due to the increase of insect-eating birds. Every fresh book on butterflies records the increasing scarcity of many species once common, and there are only a few, such as the clover-feeding clouded yellows, which are more plentiful now than in former days.

In the case of the smaller and more variable butterflies, a considerable number of varieties are figured (sometimes as many as seventeen on one plate), and we think that most entomologists who are interested in British butterflies will find Mr. South's little book a very useful supplement to any they may already happen to possess on the same subject.

W. F. K.

#### LETTER TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

#### The Latest Critic of Biometry.

MR. J. J. LISTER in his presidential address to Section D at the British Association felt it his duty to go somewhat out of his way in order to urge on biometricians "that the old adage should be borne in mind recommending that before beginning culinary operations it is advisable first to catch your hare, in other words, to make sure that the problem you seek to elucidate is sound from the standpoint of biology before bringing a formidable mathematical apparatus into action for its investigation" (NATURE, August 16, p. 400). The importance of the occasion no doubt prevented Mr. Lister from illustrating his criticism; he had much else to deal with, and he probably hoped that his words without detailed proof would have all the weight which attaches to presidential utterances. These are not made without careful thought and proper study. But in order that a criticism of this kind should be effective, biometricians need more information, and they recognised that Mr. Lister could hardly refuse to cite instances of the type of work which led to his advice.

Hoping that we might profit by Mr. Lister's caution, I wrote to him as soon as I read his paper in your columns asking for definite instances upon which we might consider how to amend our courses. He has kindly consented to

give me an illustration, so that the reasonableness of his criticism can be tried on perfectly definite and narrow grounds.

He tells me that he cannot cite a better example than a paper by Dr. Raymond Pearl which appeared recently in the Proceedings of the Royal Society dealing with a species of *Paramœcium*, and of which a fuller statement was offered for publication in the Transactions. The author's position in this paper, according to Mr. Lister, is traversed by the objection that the conjugant individuals are possibly, and indeed probably, differentiated gametes. Until this objection be met, Mr. Lister holds that the elaborate series of measurements has no cogency whatever in establishing the results which the author thinks he has obtained. Mr. Lister further believes that if Dr. Pearl were more conversant with the biological aspects of the life-history of *Paramœcium*, or less keen on the biometric aspects of the matter, he would hardly have overlooked this view.

We have here a perfectly definite charge, not a vague insinuation, which can be discussed, and I heartily thank Mr. Lister for stating it so clearly. Now as to the actual facts:—

(1) Dr. Raymond Pearl is a brilliant young American biologist who has spent much time in studying *Paramœcia* in the biological laboratories of America and Germany. He has just been called to a chair of biology at an American university.

(2) Dr. Pearl *demonstrates* for the first time in the memoir to which Mr. Lister refers that conjugant *Paramœcia* are differentiated from the non-conjugant population, a fact which his critic only considers as possible or probable.

(3) Further, Dr. Pearl *demonstrates* that among these differentiated conjugants there is an assortative mating; in other words, he shows that conjugants with certain characteristics tend to conjugate with conjugants of like characteristics.

(4) Dr. Pearl assigns with a high degree of probability the definite physiological basis for this assortative mating. He thus shows for the first time that the "physiological selection" of Romanes plays an important part in the lower living forms, and suggests the physiological origin of differentiation of species, *i.e.* all sections of a conjugant population cannot equally readily conjugate together.

Surely such problems have a very sufficient biological reality.

Dr. Pearl's paper seemed to me, as a mere biometrician, a most brilliant piece of work. That view was shared by the then Chairman of the Zoological Committee of the Royal Society, who at once passed the abstract for publication—all that lay in his power to do. The referees of the full memoir failed, so I am told, to see "the biological significance of the constants calculated" by Dr. Pearl. This appears to be Mr. Lister's condition also. The full memoir will shortly be published in *Biometrika*, so that a judgment may be formed of the value of Mr. Lister's criticism. It would have been published there originally but for two reasons. Firstly, I held the paper to be an exceptionally brilliant one, which the Royal Society ought to be proud to publish, and, secondly, that in every other branch of science papers which are very extensive, and so costly to print, naturally go to societies largely endowed for the publication of such memoirs, and not to private journals. I see no reason why biometry should be cut off from such assistance, because biology has not yet become bionomy, a transition which it must make sooner or later, as astrology passed to astronomy.

Meanwhile Mr. Lister has chosen his own ground. He cites a paper by a biologist—who happens to have studied biometric methods—as one where the hare has been cooked before it was caught, as one which deals with problems unsound from the biological standpoint. I challenge Mr. Lister to substantiate his statements:—

(1) That Dr. Pearl has neglected the differentiation between conjugants and non-conjugants.

(2) That such differentiation, whether it exists or not, makes the least difference to Dr. Pearl's investigation of whether among conjugants like conjugates with like.

(3) That Dr. Pearl has dealt with a problem unsound from the standpoint of biology.

KARL PEARSON.

#### ROYAL SOCIETY ADDRESSES.<sup>1</sup>

THE Royal Society of London is an exclusive and retired body, known of few, understood of still fewer. To most of those who are not men of science, the words "The Royal" mean the Royal Agricultural Society; many know the Royal Institution and perhaps still more the British Association; but the ancient learned body the home of which is now at Burlington House is something beyond the knowledge of most people. Nor is this to be wondered at; the Royal Society makes few efforts to make itself known, and, indeed, seems to some to do much to keep itself unknown. It gives, it is true, two public soirées, and it has its anniversary dinner; but it has managed to make the former chiefly reunions of its own fellows, and the latter, held in the darkening days of early winter "when nobody is in town," contrasts, by the paucity, nay, almost the absence, of public and distinguished guests, and the prominence of the fellows and their private friends, with the annual dinner of its neighbour the Royal Academy.

The late president of the society seems to have thought it would be well to try to make the general public better acquainted with some of the features and aims of the society, and has accordingly published, in an attractive and yet exceedingly cheap volume, richly illustrated with photographic reproductions and pleasing sketches, portions of his anniversary addresses, with the addition of a brief narrative of the early days of the society.

The topic on which he dwelt in his address of 1903, namely, the relation of the Royal Society to other scientific societies, illustrates indirectly the exclusiveness of the former, not only towards the general public, but even towards workers in science. This exclusiveness seems to have been at least encouraged by the change in management brought about in 1847. It was then decided, whether because the number chosen seemed sufficient for that day or through some prescience that it would result in the society attaining and keeping its present size, that not more than fifteen new fellows should be elected each year. Since that day the workers in science have largely increased and are continuing to increase rapidly, but the number elected annually remains the same. Hence the number who yearly join the society is a continually diminishing fraction of those who in 1847 would all have been looked upon as fit and desirable persons to become fellows. Hence also the admission to the fellowship, the gaining of the right to use the letters F.R.S., has become an honour of continually increasing value, and the allotment of the honour an increasingly important function of the society, possibly encroaching on some of its other duties. This relative narrowing of the society's body tends to accentuate its exclusiveness and emphasises its isolation from the younger workers in science. Nor is this tendency to exclusiveness counteracted by any very direct efforts to establish relations between those within and those without the narrow circle. Indeed, even within the circle itself the relations of the fellows to each other are not very close. The temple of science at Burlington House is, at each weekly Thursday service, brightened by the presence of many eager worshippers; and the fact that these are increasing in number shows that the society is putting forth the vigour of youth in one of its several great means of advancing natural knowledge. But between times the temple is well-nigh empty. What in other places would be called "weekday attend-

<sup>1</sup> "The Royal Society, or Science in the State and in the Schools." By Sir William Huggins, K.C.B., O.M., &c. Pp. xv+137. (London: Methuen and Co., n.d.) Price 4s. 6d. net.